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*Some Experiments touching Animals, made in the Air-
pump by the Persons formerly mentioned, viz. Mon-*

|| Compare Numb. 62
and 63 of these

Tracts, where many Experiments of this kind, made by Mr. Boyle, are recorded.

One day I included in the *vacuum* an *Insect* which resembles a *Beetle*, but is a little bigger; and when I perceived it to appear dead, I gave it Air again, and it soon after recovered. Then I put it in the *vacuum* again, and having left it there, for an hour, I readmitted the Air, and found, that the *Insect* needed then much more time to recover. I included it there the third time, and having left it there two daies, I gave it Air again, and saw, it needed above ten hours before it began to stir again; yet it recovered well enough this time: But having put it in again the fourth time, and left it there eight daies, it would never stir again.

Intending to try the like upon a *Butterfly*, I saw, that when I readmitted Air to it, that the top of its back, which before was much swelled, did fall in more than it should, and the *Insect* would not recover.

I also killed in the *vacuum* many Animals that breath, as *Birds*, *Mice*, *Rats*, *Rabbits*, *Cats*; and some of them I recovered by quickly giving them Air again, before the Engine was quite exhausted; but I never saw any of them revive, that had been in a perfect *vacuum*.

Monsieur *Guide* did make frequent Dissections of such Animals as we had thus killed, and observed among other things, that their Lungs fell to the bottom in water. In a little Book of his, printed of this Subject, he hath declared his opinion concerning this Experiment. He being a lover of truth, I am perswaded, he will not take it amiss, if I deliver my sentiment about it, which I think more probable than his.

He saith, that the Solidity or Closeness of the Lungs of Animals that have died in *vacuo*, comes from hence, that the Blood, which is propelled into the Lungs by the *Vena arteriosa*, doth so strongly press the *bronchi* of the *Arteria aspera*, that it expresses the Air out of them, and glues as 'twere their sides to one another. But for my part, I do not believe, that the Blood of the

vena arteriosa can thus compress those *bronchi*, because that the said blood is inclosed in its vessels, that keep and hinder it from compressing others. Yet I am not ignorant, that the things that are included in the *Oesophagus* do indeed compress the *aspera arteria*, and that the *aspera arteria* by being fill'd compresses also the *Oesophagus*, upon the account of the situation of these two conduits. But it appears not at all, that the sinallest ramifications of those *bronchi*, and of the *vena arteriosa*, are situate in the same manner; for, the *bronchi* being harder than the *arterial vein*, they will compress it more easily, than be compressed by it; and so, if you should blow them up with Bellows, they would glue the sides of that vein together, and hinder the Circulation: Which is directly contrary to the Experiment; as M. *Guide* himself observeth.

It is therefore far more probable, that, if the Lungs be compressed, that compression be made by the *pleura*, which may be swelled *within* the Breast, as the skin is swelled *without*. But it is not necessary, that the Lungs be compressed in *vacuo*, to make them subside in water. For I have divers times put pieces of Lungs and whole Lungs in the *vacuum*, and they remained there extremely swelled; but as soon as the Air was again intromitted, they became very flat and red, and sunk to the bottom in water. Which shews, that 'tis sufficient for getting the Air out of the Lungs, to render them close and red; and I have not been able to produce this effect but by means of the Exhausting Engin. For, I have left Lungs a whole night between two plates with a great weight upon them, to endeavour to express the Air out of them, but it would not succeed, and those Lungs did still float upon the water. I have also tried to make the Air re-enter into the Lungs after I had rendred them solid in the Engin, and that I found very easie; for, drawing them out from the bottom of the water, I did blow into the *aspera arteria*; and the Lungs swelled again and resumed their ordinary colour, and floated on the water. And this is that which befalls the Lungs of *Infants* new-born.

Promiscuous Experiments made in the Air-pump likewise, by the same Persons.

When I began first to keep Receivers void of Air, after the manner above-recited, I made use of *Eels-skins* to apply them close to the Cover. But I found them not proper for things that are intended to be conserved a long time, because by drying they grow springy, and this spring is capable to raise the whole pillar of Air that presses the Receiver against its Cover; and so the Air gets in between, and fills the space exhausted.

Afterwards I employed *Mutton-skin*; but that sticks yet less close than an Eel-skin: For, as soon as the external Air comes to press upon it, it makes all the water, which wetteth the skin that stands over without, enter into the emptied Receiver, and you may see little drops of water coming out of the pores of the skin that is under the Receiver; and after the water is all entred, the Air quickly gets in the same way.

At length I took a *Lambskin*, and by means thereof I have kept Receivers empty eight daies together, and never perceived it fail. Yet, for greater security, I do put *Turpentine* round about such Receivers as I mean to keep staunch a long time. Mean while, this difference betwixt the skins of Mutton and Lambs is somewhat remarkable, and confirms what Physicians say of the different Constitution of bodies in Youth and old Age. I have afterwards found, that *Paper* wetted serves as well as a Lambskin; but you must put Turpentine about it before it be dry.

One day I cover'd a Receiver, whose fourth part was fill'd with water, and the rest all empty. I put it over the flame of a Candle, and saw, that the water boiled very quickly, yet the glass not much heated; so that the water boiled near a quarter of an hour with a great ebullition, and the glass was no more than tepid. I then took it away from the flame, and saw, that the water continued a very great while boiling, and that it began again from time to time. I then believed, that the vapours, which had been raised into the Air, were recondensed by the cold, and that that made the hot water bubble up, as water usually doth when

when 'tis put into the Engine, and the Air that presses it exhausted. Meantime, I have since made the same Experiment with a *Gage*, and I did not perceive, that all the bubbles that issued out of the water, made the Mercury rise to sense.

After this, I left my Receiver exposed to the *Frost*, and I found, that the Ice which was made therein was not yet quite free from bubbles, though the water thereof had boiled in the *vacuum*, which, one would think, should have driven out all the Air: Yet the bubbles were there far less numerous than in Ice made of ordinary water. I perceived not, that the Quicksilver was much risen in the *Gage*. Afterwards I melted this Ice, and put the water abroad to freeze again, still without taking it out of the *vacuum*, and I found, that this second time it was very much freer from bubbles. The Glass did not break; but because it was somewhat conical, we could not know, whether it remained whole upon the account of its *figure*, or because that the water, which was frozen within, was freed of Air: And I could not make the Experiment with glasses of another figure, because the Frost did cease.

After this, I made *Spirit of Wine* boil in *vacuo* in the same manner I did the Water; and I saw, that it boiled much sooner. It made the Mercury rise about an inch in the *Gage*. Then I took it from the fire, and saw it continue in its boiling; and even sinking the Receiver into cold water, it thereupon boiled much more strongly. One would think, this proceeded from an *Antiperistasis*; but we have more ground to say, it came from hence, that the vapours of the Spirit were condensed, and so made the Receiver more empty; which is sufficient to make the Spirit of Wine boil, even though it were not hot. The Quicksilver did in two hours subside again, to near half a line as low as it had been. Then I put the Receiver over the flame again, and made the Mercury rise more than two inches; but then the Receiver cracked.

One day I took a *Tube of Plaster of Paris*, open at one end, and close at the other. I applied the open end to the cement as I was wont to do Receivers; and I saw it was not possible thus to exhaust it, because the Air did easily pass *through* the Plaster. I put therefore a Tube of Iron on the Engine, so as having filled it with water, the Tube of Plaster was cover'd therewith; and then

then having caused the Pump to be plied, I found, that the *Water* did pass *as easily* through the said Plaster. I therefore cover'd it with Venice-Turpentine instead of water, and then I saw, that it evacuated very well, and that nothing passed through it for the space of two hours. Then I took some Oyl very hot, and poured it over the Turpentine, which did melt by this heat and passed through the Plaster. Then I took off this Tube which was so pervaded by the Turpentine, and I saw, that that had made it transparent. Which effect is pretty like, and is to be explicated in the same manner as, [that of the little Stone called *Oculus mundi*. Thus we may be assisted by the *Weight of the Air* to make divers sorts of Glues penetrate Plaster, baked Earth, Wood, &c. And possibly those, who shall make a good number of such trials, will find their labour and pains recompenced, by giving to those materials such properties, as they never had before.

I did also put some Eggs in the *vacuum*, and one day I saw one of them break, which I had put in a small Receiver. It burst upon the very first suction : But since that time I could never make any break, though I exhausted as much as I could those Receivers wherein I had put some. You must therefore begin to crack them a little before you put them in the *vacuum*, and then they do easily break quite, and what is in the Egg riseth all into a very thick froth. I also put some of these, thus order'd, over the fire, where they boiled very easily, not being pressed by the Air, but they boiled there very long, before it began to appear that they were so boiled as to be ready to eat.

All the little bubbles that appear in *Mustard*, do swell and break in *vacuo*, and after that, the Mustard is seen to be without bubbles.

One day I included a *black Ribbon* in the *vacuum*, and then burnt it with a Burning-glass. Abundance of smoke issued out of it, which fell by little and little, and so permitted us to see the Ribbon plainly ; which appeared not at all changed. But after I had returned the Air unto it, and touched it, I found it all turn'd to ashes.

Another time I caused also some *Gunpowder* to be burnt after the same manner ; and I was much surprized to see, that it burnt grain by grain, none of the grains kindled firing those which touched. Another time when the Sun had less force, I could not

at all kindle the Powder, but I made it only boil and emit store of smoke. I had put a *Gage* in the same Recipient, by means whereof I observed, that all that smoke produced no Air; for the Quicksilver did not rise in the Tube. I noted also, that this smoak falling upon the Past-board, on which I had put the Powder, appeared yellow of the colour of Brimstone. After that, I took out the Powder that remain'd, being like a black mass, and having put it upon burning Coals, I saw it burned as doth Salt-Peter; and so it appear'd, that the sulphur was almost all exhaled.

I was willing to reiterate this Experiment, and I then saw, that the Powder, after boiling, fuming, and being kindled grain by grain, (as in the first Experiment,) at last flashes out all at once, when one hath the patience to hold the fire to it with a Burning-glass. And when the fumes are grown clearer, you may see needles of Saltpeter sticking to the sides of the Receiver.

Another time, I put the weight of 12 or 15 *grains* of Powder in a glass shaped like a Cupping glass, capable to hold 14 ounces of water, and having put fire to it, I made the Powder boil and smoak as usually. Afterwards, seeing that the corns began to crack very near one after another; I then took away the burning Concave, for fear all should be kindled together: But it was already too late; for, the corns did continue to crack longer than a second of time, and at last all kindled, though there was then nothing left to heat them but the fire which they had kept within themselves. The Receiver was lifted up above a foot high, without breaking.

Another time, I put the weight of 18 *grains* of Powder, together with a *Gage*, into a Receiver holding seven pound water; and I saw, that the Powder was more difficult to be kindled, than in small Receivers. Yet at length it was kindled altogether, and made the Quicksilver rise to the height of an inch and a half in the *Gage*; and I am very well assured, that all that Air was not come from without; for that part of the Receiver, to which the Cover is applied, had alwaies been under water.

From what I have been relating, it may be concluded, that there is a *fifth* part of Air in Gunpowder; supposing, as other Experiments do shew, that Air is about a *thou*, and times lighter than Water. For, in this Experiment, the Mercury did rise to the *eighteenth* part of the height where the Air commonly sustains

it; and consequently the weight of 18 *grains* of Powder did yield Air enough to fill the eighteenth part of a Receiver that contains seven pound of water. Now, this eighteenth part contains 49 drachms of water: Wherefore the Air, that takes up an equal space, being 1000 times lighter, weighs $\frac{1}{1000}$ of 49 drachms, which is more than $3\frac{1}{2}$ grains. It follows therefore, that the weight of 18 *grains* of Powder which I employed in my Experiment, contained more than $3\frac{1}{2}$ of Air, which is about the fifth part of eighteen grains.

It may also be calculated, how many times this Air hath been compressed in the Powder: But this calculation is more uncertain than the former, because we know not, whether this Air took up more or less than the fifth part of the space which the Powder possessed. But yet 'tis certain, that, though it had even taken up *three fourths* of the whole room of the Powder, and that the fourteen grains of the other matter had taken up no more than the one remaining fourth part, still this Air would have been compressed about three hundred times. To calculate this, I suppose, that the space of a Cubic foot can hold only 72 pound of Gunpowder, which do contain more than 14 pound of Air, by the foregoing *calculus*; which quantity of Air is therefore found inclosed in the *three fourths* of a Cubic foot. Now, this space doth usually contain but about six drachms of Air: Wherefore, to make it hold fourteen pound of Air, which is near three hundred times six drachms, it must needs be, that that Air be compressed near three hundred times.

There is reason to believe, that this Compression is much greater, because a Cubic foot can hold much more than seventy two pound of Powder, and because also that the *fifth* part of the weight must not, in appearance, possess alone the *three fourths*, and all the rest take up no more than *one fourth* of the space possessed by all the Powder.

I should therefore make no difficulty to believe, that all the effect of Gunpowder comes from the Air which is compressed therein, and especially in the Saltpeter; for I have not observed, that Brimstone yields Air. Possibly also we may find in time, that all other Fulminations, Ebullitions and Fermentations, that make such surprizing motions, are nothing else but Air compressed expanding it self.